

1509/541  
R E M A R K S

ON THE

O B S E R V A T I O N S

MADE IN THE LATE

V O Y A G E

T O W A R D S

T H E N O R T H P O L E,

For determining the Acceleration of the PENDULUM, in Latitude  $79^{\circ} 50'$ .

By SAMUEL HORSLEY, LL. D. Sec. R. S.

IN A LETTER TO

The Hon. CONSTANTINE JOHN PHIPPS.

L O N D O N:

Printed for B. WHITE, in FLEET-STREET; and P. ELMSLEY, in the STRAND.  
MDCCLXXIV.

R E M I A R S

ON THE

O B S E R V A T I O N S



MADE IN THE LATE

Y O A G E

TOWARDS

T H E N O R T H P O L E

BY

WILLIAM G. B. HARRIS

OF

THE UNIVERSITY OF CAMBRIDGE

AND

OF THE ROYAL OBSERVATORY, GREENWICH

---

TO THE HONOURABLE

CAPTAIN PHIPPS.

DEAR SIR,

I Am favoured with your obliging present of your Voyage towards the North Pole, for which I return you my best thanks.

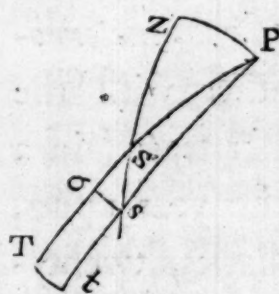
Mathematicians are no less indebted to you than mariners, for the attention which you have given to every object of scientific enquiry, though but remotely connected

with the nautical art, which that singular voyage presented. I have perused with particular attention the account of the observations of the going of the pendulum in latitude  $79^{\circ} 50'$ . and shall give you my remarks without apology, which it would be the highest injustice to you not to suppose unnecessary, after the pains you have bestowed upon the observation, and the minuteness and fidelity with which you have detailed all the circumstances of it, as well as the steps of the subsequent calculations.

I am inclined to believe that the gain of the pendulum must have been very nearly what you reckon it. But the evidence of this rests entirely upon the comparison with the WATCH, and the fix altitudes of the sun taken with the astronomical quadrant for determining the loss of the watch. For the exact agreement which you think you find between the gain of the pendulum as resulting from the comparison with the watch, and as deduced from the observation of the sun's return to the vertical wire of the equatorial telescope, is *imaginary*. The appearance of agreement arises entirely from an error in the computation of the retardation of the sun's return; and when this error is set right, the watch and the observation will be found to differ considerably.



The interval between the time when the sun's western limb touched the vertical wire on the 16th day of July, 1773, and the time of the return of the same limb to the vertical wire on the day following, which your computer hath reckoned  $24^h 0' 49''$ , 5, could be no more than  $24^h 0' 14''$ : for, a small change in the sun's declination is to the corresponding change in the hour-angle (not, as your computer states it, as the cosine of the latitude of the place of observation, but) as the cosine of *the sun's declination* to the tangent of the angle contained between the circle of declination and the vertical circle: for, let Z be the observer's zenith, P the pole of the world, ZS the vertical circle drawn to the sun, S the place of the sun upon the vertical circle at the time of the first observation, s its place



upon the same vertical at the time of the second observation. Draw the great circles ZP, PS, Ps; round the pole P, describe a small circle through s, sσ, and continue the arcs PS, Ps, till they meet the circumference of the equinoctial circle in T, t: now Sσ is the change of declination in the interval of the two observations; SPs is the corresponding change of the hour-angle ZPS, and Tt is the arc of the equinoctial, which measures the angle SPs. I say that  $Sσ : Tt = \sin. PS : \tan. PSZ$ .

For the evanescent triangle  $S s \sigma$ , is to be considered as a right-lined triangle right-angled at  $\sigma$ . Therefore

$$S \sigma : \sigma s = \text{radius} : \text{tang. } s S \sigma, \text{ or } P S Z.$$

And, by the known properties of the sphere,

$$\sigma s : T t = \text{fin. } P \sigma : \text{radius.}$$

*Ex æquo perturbatè*  $S \sigma : T t = \text{fin. } P \sigma : \text{tang. } P S Z$ . That is, because  $P \sigma$  and  $P S$  are so nearly equal, that the sine of the one may without sensible error be taken for that of the other,  $S \sigma : T t = \text{fin. } P S : \text{tang. } P S Z$ . Q.E.D.

Your computer affirms, that

$$S \sigma : T t = \text{fin. } Z P : \text{tang. } P S Z;$$

and to support this assertion he refers to Cotes *Æstimatione errorum*, Theor. 35.

Mr. COTES's most elegant treatise, *De Æstimatione errorum in mixtâ Mathesi*, is published, together with the *Harmonia Mensurarum*, and some other lesser tracts, in a quarto volume, printed at Cambridge in the year 1722. It contains in all, four lemmata, three corollaries, twenty-eight theorems, and two scholia. I suppose, therefore, that theor. 35 is an error of the press. The theorem that ought to have been referred to, is the *thirteenth*: for, when the sun returns for the second time to the vertical circle  $Z S$ , and comes upon it at  $s$ , when before it had been at  $S$ , the triangle  $Z P S$  is changed into  $Z P s$ : that is to say, the side  $Z S$  is changed into  $Z s$ , the angle

$Z P S$

$ZPS$  is changed into  $ZPs$ , and the side  $PS$  into  $P s$ , and the angle  $PSZ$  into  $P s Z$ ; and the only two parts of the triangle which remain unchanged, are the side  $ZP$ , which is the complement of the latitude, and the azimuth-angle  $PZS$ : so that the thing to be investigated is this; when one side of a spherical triangle ( $ZP$ ) and one of the two angles adjacent to that side ( $Z$ ) remain of a certain constant quantity, what change must the other angle ( $P$ ) adjacent to the unchanging side ( $ZP$ ) undergo, in order that the side ( $PS$ ), opposite to the unchanging angle ( $Z$ ), may be changed in its magnitude by a given small quantity  $S\sigma$ ? This question is answered by Mr. COTES's thirteenth theorem, and by no other proposition in his whole tract. And in that thirteenth theorem, Mr. COTES affirms, as I do, that the arc which measures the variation of the angle adjacent to the unchanging side, is to the variation of the side opposite to the unchanging angle, as the tangent of the angle opposite to the unchanging side to the sine of the side opposite to the unchanging angle. And his demonstration is derived from the very same obvious principles.

Instead of the proportion therefore which your computer states, make the following: As the sine of  $PS$ , *i. e.* as the cosine of sun's declination, to the tangent of  $S$ , so is the change in declination to the change in the hour-angle  $2' 5'', 1$ ;

Which

|                                              |    |         |                                              |         |
|----------------------------------------------|----|---------|----------------------------------------------|---------|
| Which turned into time gives                 | 0  | 8,3     | Gain of pendulum upon watch                  | 79,3(a) |
| The change of the equation was               | 0  | 5,4     | Loss of watch by mean of six solar altitudes | 5,5     |
| Hence true interval of the two transits      | 24 | 0 13,7  | Gain of pendulum upon mean time              | 73,8    |
| Interval by pendulum                         | 24 | 2 4,5   | Gain by cold                                 | 2,72    |
| Therefore the gain of the pendulum should be | 0  | 1 50,8  | Gain by increase of latitude                 | 71,08   |
| Gain by cold                                 |    | 2,72    |                                              |         |
| Gain by increase of latitude                 |    | 1 48,08 |                                              |         |

Gain by increase of latitude according to observation 1 48,08  
Ditto by watch 1 11,08

Difference 0 37,00

Thus the observation gives the gain of the pendulum 37" more than the watch. But as the watch went so well during the whole voyage, as its loss in these twenty-four hours was ascertained by six altitudes of the sun, and as the gain now given by the watch agrees so nearly with the result of the subsequent comparisons at SMEERENBERG POINT, I have no doubt but that the error lies entirely upon the observation of the second transit. I suppose the telescope, from some unperceived cause, had shifted its azimuth; which is the more probable, as it does not appear that any means were used to verify the position of the instrument. Perhaps

(a) The interval between the two transits in mean time was 24<sup>h</sup> 0' 13",7. And the watch having lost in that time 5",5, the interval in time of the watch was 24<sup>h</sup> 0' 8",2 : which is more than 23<sup>h</sup> 26' 42",5 (the time in which the pendulum had gained 77",5 upon the watch, vid. Tab. A.) by 33' 25",7. But as 23<sup>h</sup> 26' 42",5 to 33' 25",7 (*i. e.* as 211 to 5 very nearly) so is 77",5 to 1",8. Therefore 77",5 + 1",8, or 79",3, will be the gain of pendulum upon watch in the whole interval between the two transits.

the

the situation upon the small rocky island afforded none (*b*).

You finish your article relating to the pendulum with saying, "that these observations give a figure of the earth nearer to Sir Isaac Newton's computation, than any others which have hitherto been made;" and then you state the several figures given, as you imagine, by former observations, and by your own. Now it is very true, that *if* the meridians be ellipses, or, *if* the figure of the earth be that of a spheroid generated by the revolution of an ellipse, turning on its shorter axis, the particular figure, or the ellipticity of the generating ellipse, which your observations give, is nearer to what Sir ISAAC NEWTON saith it should be, if the globe were homogeneous, than any that can be derived from former observations. But yet it is not what you imagine. Taking the gain of the pendulum in latitude  $79^{\circ} 50'$  exactly as you state it, the difference between the equatorial and the polar diameter, is about as much less than the Newtonian computation makes it, and the hypothesis of homogeneity would require, as you reckon it to be greater. The proportion of 212 to 211 should indeed, according to your observations, be the pro-

(*b*) Captain Phipps, in a letter to me of the 15th of September, says, "You were right in supposing that the situation of the telescope did not admit of any means of verifying its position."

portion of the force that acts upon the pendulum at the poles, to the force acting upon it at the equator. But this is by no means the same with the proportion of the equatorial diameter to the polar. If the globe were homogeneous, the equatorial diameter would exceed the polar by  $\frac{1}{230}$  of the length of the latter: and the polar force would also exceed the equatorial by the like part. But if the difference between the polar and equatorial force be greater than  $\frac{1}{230}$ , (which may be the case in an heterogeneous globe, and seems to be the case in ours,) then the difference of the diameters should, according to theory, be less than  $\frac{1}{230}$ , and *vice versa*.

I confess this is by no means obvious at first sight; so far otherwise, that the mistake, which you have fallen into, was once very general. Many of the best mathematicians were misled by too implicit a reliance upon the authority of NEWTON, who had certainly confined his investigations to the homogeneous spheroid, and had thought about the heterogeneous only in a loose and general way. The late Mr. CLAIRAUT was the first who set the matter right, in his elegant and subtle treatise on the figure of the earth. That work hath now been many years in the hands of mathematicians, among whom I imagine there are none, who have considered

considered the subject attentively, that do not acquiesce in the author's conclusions.

In the second part of that treatise\*, it is proved, that putting  $P$  for the polar force,  $\Pi$  for the equatorial,  $\delta$  for the true ellipticity of the earth's figure, and  $\varepsilon$  for the ellipticity of the homogeneous spheroid,

$\frac{P-\Pi}{\Pi} = 2 \varepsilon - \delta$ : Therefore  $\delta = 2 \varepsilon - \frac{P-\Pi}{\Pi}$ ; and therefore, according to your observations  $\delta = \frac{1}{111}$ . This is the just conclusion from your observations of the pendulum, taking it for granted, that the meridians are ellipses: which is an hypothesis, upon which all the reasonings of theory have hitherto proceeded. But plausible as it may seem, I must say, that there is much reason from experiment to call it in question. If it were true, the increment of the force which actuates the pendulum, as we approach the poles, should be as the square of the sine of the latitude: or, which is the same thing, the decrement, as we approach the equator, should be as the square of the cosine of the latitude. But whoever takes the pains to compare together such of the observations of the pendulum in different latitudes, as seem to have been made with the greatest care, will find that the increments and decrements do by no means follow these proportions; and in those which I have examined, I find a regularity in the deviation which little re-

fembles the mere error of observation (*b*). The unavoidable conclusion is, that the true figure of the meridians is not elliptical. If the meridians are not ellipses, the difference of the diameters may indeed, or it may not, be proportional to the difference between the polar and the equatorial force; but it is quite an uncertainty, what relation subsists between the one quantity and the other; our whole

(*b*) This will best appear from the following table, which shews the different results of observations made in different latitudes. The three first columns contain the names of the several observers, the places of observation, and the latitude of each. The fourth shews the quantity of  $P-\Pi$  in such parts as  $\Pi$  is 100000, as deduced from comparing the length of the pendulum at each place of observation, with the length of the equatorial pendulum as determined by M. Bouguer, upon the supposition that the increments and decrements of force, as the latitude is increased or lowered, observe the proportion which theory assigns. Only the second and the last value of  $P-\Pi$  are concluded from comparisons with the pendulum at Greenwich and at London, not at the equator. The fifth column shews the value of  $\delta$  corresponding to every value of  $P-\Pi$ , according to Clairault's theorem:

| Bouguer.           | Equator.           |       | $P-\Pi$ | $\delta$        |
|--------------------|--------------------|-------|---------|-----------------|
|                    |                    | 0     |         |                 |
| Bouguer.           | Porto Bello.       | 9—34  | 741,8   | $\frac{1}{784}$ |
| Green.             | Otaheitee.         | 17—29 | 563,2   | $\frac{1}{326}$ |
| Bouguer.           | San Domingo.       | 18—27 | 591,0   | $\frac{1}{358}$ |
| Abbé de la Caille. | Cape of Good Hope. | 33—55 | 731,5   |                 |
|                    | Paris.             | 48—50 | 585,1   | $\frac{1}{351}$ |
| The Academicians.  | Pello.             | 66—48 | 565,9   | $\frac{1}{329}$ |
| Captain Phipps.    |                    | 79—50 | 471,2   | $\frac{1}{251}$ |

By this table it appears, that the observations in the middle parts of the globe, setting aside the single one at the Cape, are as consistent as could reasonably be expected; and they represent the ellipticity of the earth as about  $\frac{1}{345}$ . But when we come within ten degrees of the equator, it should seem that the force of gravity suddenly becomes much less, and within the like distance of the poles much greater than it could be in such a spheroid.

theory,

theory, except so far as it relates to the homogeneous spheroid, is built upon false assumptions, and there is no saying, what figure of the earth any observations of the pendulum give.

I flatter myself that you will take these strictures in good part, as the only motive which induces me to trouble you with them, is one which I am persuaded is a ruling principle with yourself, a regard to truth. Had your account been less circumstantial, the mistakes which have crept into the calculations might not have been so easily detected.

I have the honour to be, Sir, with the greatest respect and esteem,

Your most obedient and

most humble Servant,

SAMUEL HORSLEY.

P O S T-

## P O S T S C R I P T.

THE time that the sun's diameter takes to cross the vertical wire, hath no connection with the principal object of your enquiry: the rule, however, which is given in the note, p. 161, is not a true one. The true rule might be deduced from Cotes Theorem 21. But it may be come at in a much easier way, and is this:

As the cosine of the angle (S), contained by the vertical circle and circle of declination, to the radius, so is the sun's diameter in time to the time sought. You will find this rule, and a very simple demonstration of it in Mr. De La Lande's Astronomy, article 596, first edition. The sun's diameter in time on the 16th July, 1773, was  $135''{,}2$ ; whence the time of its passing your vertical wire must have been  $137''{,}6$ . The reason that your computer, though working by a false rule, hath come in this point very near the truth, is, that at the time of your observation the sun's altitude was very nearly equal to his declination, and the cosine of altitude very nearly equal to the cosine of declination; and consequently the proportion of *cosin decl.*  $\times$  *cosin S* to *radius*  $\times$  *cosin alt*, which your computer takes for the proportion between the sun's diameter in time and the time sought,

fought, is, in this particular instance, very nearly the same with the proportion which universally obtains between those times, viz. that of  $\cos \sin$ . S to the radius. But had the observation been taken at a time when the sun's altitude had been very different from his declination, the error of his rule would have discovered itself; of which you may easily satisfy yourself by trial, if you think it worth while.

*In justice to Captain PHIPPS I think myself obliged to inform the public, that the foregoing letter is published with his consent; and that I have his authority to say, that the calculations, which have given occasion to it, namely that of the retardation of the sun's return to the vertical wire, and that of the time which the sun's diameter should take to pass the vertical wire, were both made by Mr. ISRAEL LYONS.*

F I N I S.

